Biogeosciences

Discussions

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Interactive comment on "Different sensitivities of litter decomposition and nutrient release to ultraviolet radiation" *by* Weiming Yan et al.

Anonymous Referee #1

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I have read with interest the manuscript of Yan and co-authors titled "Different sensitivities of litter decomposition and nutrient release to ultraviolet radiation". This meta-analysis synthesized global data of UV changes on litter mass loss, C, N and P release and lignin degradation during decomposition. The authors also discussed the magnitudes of UV treatments on litter decomposition among various experimental conditions, litter types, durations and precipitation levels. In general, I found this paper to not be well written, especially the discussion. I also found some terminologies and presentations to be confusing in some cases. But I think the data are scientifically valid and interesting to the wider community, so I think this manuscript is suitable for publication in Biogeosciences after reversion. Here, I provide some comments which the authors

may find useful.

General comments: A large part of the results were presented again in the Discussion. A rewriting of the results should allow to extract more explicitly the meaning of the findings avoiding the need to be repeated in the discussion. The authors have made many classifications, i.e., lab/field, litter types (although not very correct and could be re-divided), durations and MAP levels, but some interesting findings were not presented. For example, why UV enhancement and attenuation had opposite effects on mass loss vs. N/P release (Fig. 1)? Why UV changes had strong effects on litter N content and C/N and lignin/N ratios (Fig. S4)? The authors made a very simple regression result in Fig. S5, which, however, I think cannot help us to answer the above two questions. Instead, for the Fig. S5, why not to try to separate the control and treatment data because UV treatment did had very strong influence on litter N content and associated ratios, and this may help us to make insightful discussion. I remember that the photo-degradation of litter decomposition was found in arid grassland (i.e., Austin 2006 Nature). Yes, the authors compared the decomposition rate and MAP, and from that figure, the regression results were interesting, particularly in arid areas with low precipitation. However, it was much different when the MAP reached at 1400 mm, so I think the regression results can be presented as bars with different categories (i.e., MAP ranging from 0-50 mm, 50-100 mm, etc.). Of course, the previous Figure 5 can be presented in Supplementary Information. Following the above question, photo-degradation of litter decomposition may be observed in grasslands in arid ecosystems in previous years, but in recent years, there were many studies conducted in forests. Therefore, why not divide the litter type to more specific classifications (i.e., grass, herb, broad-leaved and needle foliage) corresponding to the ecosystem types (grasslands vs. forests)? By the way, I think just use "herb" in grasslands was not correct and the authors should carefully distinguish grass and herb.

Detailed comments: Line 30. "weight loss"? And why not "mass loss"? The latter one is more widely used in litter decomposition studies. Line 222. The authors

declare that "UV-(A+B) attenuation... but showed LITTLE effect..."; however, "UV-B enhancement... showed SIGNIFICANT effect" (line 221). I think the authors should change the presentation. In fact, the RR for UV-(A+B) attenuation was less than -0.25, but that for UV-B enhancement was only 0.04, so why did the authors say UV-B enhancement had SIGNIFICANT effect whereas UV-(A+B) attenuation has LITTLE effect? In fact, the RR for UV-B enhancement maybe not significant (overlap with zero) if the sample size was smaller. Lines 223-225. The authors declared that the RR was greater for k decay compared with mass loss, but I think the authors should treat the k decay and mass loss results with caution. There are at least two reasons: First, both the k decay (assumed that the exponential models were used) and mass loss reflect one thing. Second, the sample size for UV-B attenuation on k decay was very small (n=4), so its confidence was not strong, and this result may be excluded in some cases. Therefore, I think using other sentences (i.e., Similarly, k decay...) would be better than "... greater than ...". Line 225. Why the authors did not present some results on N and P release directly? That will be very interesting because it seems that UV-B enhancement and attenuation showed opposite effects on N/P release relative to on mass loss/k rate. And then C and lignin. Line 225. I think "no effect" was not a very good word if we have other choice. Why not "the effects were not significant"? Line 257. "UV enhancement had NO effect on the weight loss in the first four months"? I am confused why some RRs were not significant (marked in gray) but the 95% CI did not overlap with zero. This problem can be found in many figures. Please check it. Figure 267-268. What is "control treatment"? "Control + treatment"? From Figure 1, UV treatment had very strong influence on k rate, so have you tried to compare the relationships between MAP and k rate under control and treatment conditions separately? Just like Figure 6. Lines 275-278. I am confused that how can the authors conclude that ... was more sensitive than .... The slopes >1? Or compared with the 1:1 lines in Figure 6? Lines 284-289. It seems that this sentence was redundant here because this has been presented in the Introduction. We should focus on the most important findings, and some general sentences, i.e., litter decomposition is a complex process regulated

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by both biotic and abiotic factors, were not very interesting for readers. Lines 292-295. Just like the suggestion mentioned above, the sentence "UV enhancement had ... lead to a decrease" has been introduced in the Introduction section, so we do not need to repeat it again here. Line 302. I think "litter decomposition" should be replaced by "mass loss" here. As you declared (but I suggest to delete it) that "litter decomposition is a complex process" and this process includes many sub-process, i.e., mass loss we observed, C structure breakdown, release of N, P and other nutrients, etc. Therefore, UV treatments had opposite effects on mass loss vs. N/P release and not litter decomposition vs. N/P release. Line 310. I think the authors should discuss the sample size for remaining C, and not only "a different regulatory mechanism". In meta-analysis, the collected data have strong influence on our conclusion, which may be biased if the sample size is too small. This is a potential uncertainty in meta-analysis.

Figures. The authors should note that what are the meanings of the error bars, e.g., 95% CI. Figure 1. Maybe there was a small mistake for the UV-B attenuation (triangle) for "k decay" in Figure 1. Generally, the difference can be considered as significant if the 95% CI did not overlap with zero. However, for the UV-B attenuation for "k decay", the 95% CI did not overlap with zero, but it was gray and not black, although the sample size was very small (n=4).

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